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No observation of modern Physical Astronomy is more striking in its conception, than that which attempts to determine the motion of a celestial body, by the altered wave-length of its light, and none has attracted more general attention. It is popularly understood, I think, that the proper motion of certain stars in the line of sight, has been thus completely demonstrated, but those particularly engaged in such studies know how far astronomers have till very lately been from the certainty attributed to them. Only last April, Father Secchi presented a communication to the Comptes Rendus† in which he recalled the denial on theoretical grounds, by Van der Willigen, of the possibility of any such observation, pointed out the extreme danger of instrumental error in such spectroscopic work, and gave experiments of his own, to show that its causes were so numerous and so subtle that it was difficult to be certain of any result.

Without discussing the replies, which aim to show that lately precautions have been taken which it may be hoped are effective, it may be observed that there is another mode of investigating the reality of the phenomena, not liable to as many difficulties as in the case of the stars, though presenting a formidable one of its own. I mean, of course, the observation of the different wave-lengths of light coming from the east and west limbs of the sun, which, owing to that body's rotation on its axis, have equatorial velocities that together make up nearly two and one-half English miles a second. This speed, enormous in itself, is most insignificant compared with the velocity of light, and this relative smallness constitutes the difficulty in

* Communicated to the meeting of the National Academy, held in Washington, April, 1877.

† C. R., tome lxxxii, pp. 761–812.

attempting the solution of the problem by means of the sun, for the whole displacement due to it, is (as Professor Young has remarked in illustration), but one-seventy-seventh of the distance between the D lines, or between one-twelfth and one-thirteenth of one division of Angström's scale. Zöllner, Secchi and Hastings have believed that they, nevertheless, detected a change in the refrangibility of the light, and Vogel* using Zöllner's reversion spectroscope obtained a displacement of from ·08 to 0·15 of one of Angström's units. In August last, Professor Young gave the results of his own measurements with one of Mr. Rutherfurd's gratings, showing an equatorial velocity of $1\cdot^{m}42$. Professor Young was unable to find any displacement of the atmospheric lines. This last research† being much more systematic than its predecessors, and given in satisfactory detail, has turned the weight of scientific opinion in favor of the view, that the change due to motion of the luminous body is fairly proven. It can hardly, however, be deemed superfluous to still offer upon so important a question, the results of an independent method of measurement, and one which renders errors from instrumental displacement, on the danger of which so much stress has been deservedly laid, in the sense in which the word is here used, not only unlikely but impossible.

In the course of a research upon the selective absorption of the solar atmosphere, I arranged in 1875, means for comparing homogeneous lights from different parts of the disc. The apparatus was too complex for description here, but it consisted essentially, in the provision of two pair of right-angled prisms of total reflection, so disposed in connection with a spectroscope, that the spectra could be formed side by side, of light from different parts of the sun, and of a photometric apparatus by which the relative intensity of the lights at different parts of these spectra could be compared. The results of this research, with an improved form of the instrument, will I hope be soon ready for publication. It was not intended, primarily, for the comparison of individual spectral lines, for which purpose optical arrangements, not very essentially different, had already been used by Lockyer, Hastings and perhaps others, but the investigation of Professor Young suggested to me another and cognate method of testing the principle of Doppler, to which this apparatus is especially applicable. The theory of the proposed method is very simple. Let two spectra be formed side by side, the one of light from one edge of the sun, the other of light from a point 180° distant. The instrument being in adjustment, if these points be in the neighborhood of the solar poles which are relatively at rest, all the lines

* Beobachtungen auf der Sternwache zu Borhkamp.

† American Journal of Science and Arts, vol. xii, Nov., 1876.

will be continuous in both spectra. But if the instrument is rotated till the light comes from points on the eastern and western sides of the sun, which are in relative motion, not only will the solar lines be discontinuous, in the two spectra; as though the one receiving light from the advancing or eastern side had been slid past its neighbor toward the violet; but any mal-adjustments of the instrument, which simulate this effect, can be with certainty detected by a means to be shortly described. The actual quantity by which we may anticipate from theory that the spectra are displaced, is, as has been observed, extremely small, and to produce the desired result we need not only very great dispersion in the spectroscope, but very delicate workmanship in the cutting and mounting of the prisms which are to bring the light to the slit, and in general it will be evident that more than common skill is to be desired of the instrument maker.

I wish to acknowledge the obligations I am under in these respects to Mr. F. Walther, of Philadelphia, the optician, and to Mr. William Grunow, the maker, of New York. Not to dwell on those difficulties of detail which always come between the conception and its embodiment in practice, I may say that more than six months have passed in experiment and modification till the instrument has attained its present form, in which I have been finally able within the past few days to see my anticipations justified. That it has been possible to me to undertake this research at all at present, is due to Mr. Rutherfurd, who has given me choice specimens of his gratings, which are so generally known and valued, that it is unnecessary that I should describe them.

It is desirable that a very clear mental picture should be present to the observer, of the *amount* of displacement he is to expect, for though it is well that he should be ignorant, if possible, of the anticipated direction, this knowledge of the amount will prevent him at the outset from confusing apparent displacements due to mal-adjustment of the instrument with those to which his attention should be directed.* I have taken advantage of a table prepared by Professor Pickering, showing the

* The following method of statement appears to present material for such a mental picture. In the 3d spectrum of the 17280 line grating the D lines, viewed under the magnifying power most favorable in my instrument to clear vision, appear like two sharply-defined parallel lines $\frac{1}{8}$ inch apart viewed by the naked eye at the distance of 10 inches. One of the units of Angström's scale appears to fill rather more than $\frac{1}{80}$ inch at the same distance. The displacement here due to rotation is as is remarked rather more than $\frac{1}{12}$ or ·08 of a unit, or, referred to the distance of distinct vision, very near $\frac{1}{600}$ of an inch. It is known that we can with the naked eye distinguish $\frac{1}{3600}$ of an inch or less, in the form of discontinuity of two lines on a vernier plate, which is essentially our present case. Owing to defective light and other causes, we cannot do quite this on the sun, as yet, but can still count on detecting by this method a discrepancy of somewhat less than ·03 units, which again is less than half the amount in question.

results of different instruments on the fine lines of the E group, to compare mine with them, and I observe that I can discriminate more, and more delicate lines, with either the 8,000 or 17,000 line grating than observers have been able to do with the most powerful spectroscopes of the common construction. Where Angström, using a grating of Nobert's, has delineated twelve lines, the grating supplied me by Mr. Rutherfurd shows thirty-one, and one of these pairs, though separated by but distances of one-sixth of a unit of Angström's scale, is yet clearly divided. The displacement to be expected from the solar rotation, under favorable circumstances, is about one-half of this, and hence if it exist, it is clearly within the power of the instrument to exhibit, since the two spectra are formed in juxtaposition as nice as that of a vernier plate to its circle, a circumstance under which, as every one knows, the eye judges of the continuity of lines with the most accuracy.

The instrument itself consists of the grating, with a collimating and observing telescope, each of one and one-quarter inch aperture and fourteen and one-half inches focus, with positive eye-pieces magnifying from ten to thirty times. The slit is constructed so that its jaws are bevelled to knife-edges, where they meet, at the outer surface, and these owing to the excellent workmanship, still close so as to admit no light when held up against the sun. Over this slit, with their bases almost in contact with it, and their united edges crossing it at right angles, are the first pair of reflecting prisms. They are cut from the same piece of glass, and so fitted that their junction-line shows no more in the spectrum than a particle of dust, and in fact, the division between the two spectra is with difficulty distinguishable from the ordinary dust lines. The other two prisms slide independently on ways, with verniers reading to thousandths of an inch, permitting them to be put in any part of a solar image of from one and one-half inch, to five inches in diameter. Over them is a hood, carrying a screen, which receives the image projected by the equatorial telescope. This focal image is nearly one and three-quarters inches in diameter, and any two selected portions of it pass through adjustable apertures in the screen. By the construction of the instrument they are equally distant from the optical axis, and in every other optical condition, as far as possible, similar, except as they may differ at their origin in the sun itself. It shows the difficulties of the method of research successfully employed by Mr. Huggins, and at Greenwich, that when the instrument is turned directly on the sun (i. e. so that each spectrum receives light from all parts of the sun's disc), the two sets of spectral lines will not ordinarily be continuous. Theoretically they should be, practically, we find they are not, owing to numerous latent causes

of disturbance. A touch on the prisms, a movement of the slit, an adjustment of the eye-piece, will ordinarily disturb one spectrum relatively to the other, by a minute amount. But the whole change we are seeking is of a minuter order still; how then, can we discriminate it with certainty? In our ability to do this lies the advantage of the method I describe, which, granting sufficient dispersive power, makes impossible the instrumental error that has been dwelt on by Secchi and others with justice, as shaking confidence in the result. To see how we are authorized to use this word "impossible," let us bear in mind that the solar spectrum consists of two distinct kinds of lines, one caused by absorption in the solar, the other by absorption in the terrestrial atmosphere. These latter being formed by light from all parts of the sun are independent of its rotation.

The prisms are adjusted, till, on looking on the sun directly, the lines are all continuous in both spectra, then the instrument is put in the telescope and the slit placed at such a position-angle that the light in spectrum A comes from the vicinity of the north solar pole, that in spectrum B from the south. On looking in, we see a very long and narrow spectrum, filled with dark lines and exhibiting the chromospheric lines on both sides. It is divided by what appears to be a fine dust line, in two exactly corresponding parts, and is in reality two distinct spectra, as we see by the opposed chromosphere lines, but as the sources of light for both spectra are relatively at rest, all the dark lines are still continuous. But now (without disturbing any adjustment), revolve the whole 90° about the optical axis passing through the center of the solar image, so that spectrum A is formed by light from the eastern or advancing edge of the sun; spectrum B by light from the western or retreating one. A curious change has taken place. By a very minute but perceptible quantity, spectrum A appears to have been slid past its neighbor, toward the violet end, so that every solar line in the first is "notched" at its junction with the second, while at the same time, the telluric lines are as unaltered as the fixed lines of a micrometer web would be, by moving a scale about in the field. The effect is the same as though the spectra were tangible things, like two engine-divided scales, whose numerous delicate divisions (represented by the solar lines), were all in exact juxtaposition a moment before, and are all now just perceptibly displaced, as when a vernier plate is moved till a coincidence is made at a new stroke on the limb.

Moving the instrument 90° more, we come again into the axial line of the sun, and the coincidence should return; with still 90° more we are again in the equator, but now spectrum A is formed by light from the western edge, and this time it is

moved the other way, as if it were a scale which had been slid by a very slight but distinctly perceptible amount toward the *red* end; while still the telluric lines retain their continuity, assuring us that no mal-adjustment has occurred.

It will be admitted that this change is, if real, excellent experimental evidence that the wave length is virtually different in light from the eastern and western limbs, as theory predicts. For, granting that the instrument is mal-adjusted in any unknown way or degree, any *instrumental* cause will affect solar and telluric lines alike, and we may in fact defy ingenuity to suggest an error of adjustment, which will modify one and not the other.

For the sake of clearness, I have assumed that we start with all the lines continuous in both spectra; in practice this condition is not easily assured: commonly some lurking error, will, without especial pains, cause them to appear broken upon a fixed source of light; but we disregard this, and consider, as we bring the instrument into new positions, only the *difference* of displacement of the solar and telluric lines. The simultaneous observations of this difference, in each of two spectra, is the essential condition relied on, not only in theory but in actual work.

It will be remembered that many lines in the spectrum are only seen when the sun is low. These are clearly due to absorption in our atmosphere. Many thousands, as we know, are due to absorption in the sun's atmosphere. There remains a large number of lines not coincident with any we produce at the electrodes of our battery, and always present in the spectrum. Of these we do know that they are either caused by the sun's atmosphere or ours, without always knowing which, for these can only be inferred to be telluric from their growing stronger as the sun sets,* and this, though easily determined in the case of a single line, becomes a task of great labor where we deal with thousands. It is evident, however, that after having used known telluric lines to determine the fact that the refrangibility of solar lines only is altered, we can reverse the process, and classify unhesitatingly hereafter, all lines as telluric which are unaffected by the changes that compel others to betray their solar origin. To merely see these two spectra with clearness, then, is to be enabled to pick out the telluric lines from the others, as though they were mapped before us. They are mapped in fact, and it becomes, under the proper conditions, a matter of simple inspection to determine them.

I hope then, it will not seem too assuming a title, if I speak of this as a new method in solar spectral analysis.

I have only to add, that in all my trials of this method, I have

* Or by experiment on artificial lights viewed through intervening atmospheres.

constantly so arranged each experiment, that I remained in intentional ignorance as to which spectrum came from the eastern, and which from the western limb, until I had determined the point by the different behavior of these solar and terrestrial lines, which I have been able to do thus far correctly in every instance. I believe, in fact, that the effect under proper conditions is so marked that the observer hereafter need not take pains to guard against the unconscious bias of his measurement toward a desired result. I have, in these preliminary experiments, however, thought it necessary to take precautions that no such prepossession, if it existed, might influence me unawares.

I ought to add that the proposed method is considerably limited, not only by the need of very powerful instruments (for these can be supplied), but by the need of very good observing weather, which unfortunately we have not at command when we want it.

Useful tests of the desired condition are the duplication of such lines as 1529 of Kirchoff's scale, and the more celebrated "1474," known as a close double since Professor Young's duplication of it last year.

I have not yet made any measurements of the displacement by this method, which appears to me as thus far described to be less adapted to quantitative results than that already employed by Professor Young, but capable by the use of the micrometer of giving exact numerical data. I ought to add, however, that Professor Rood, of Columbia College, has offered the valuable suggestion, that by the use of a double refracting prism, we can cause the spectra to overlap, thus not only overcoming some mechanical difficulties which I have not enlarged on, but obtaining at little trouble all the advantage of micrometric measurement. I regret that the instrument reached me so late that I have not yet been able to give a trial to this, from which I hope excellent results. Nor have I been able to introduce photography with other contemplated improvements, such as the simultaneous use of both right and left spectra of the same order, thus doubling the displacement; and the use of three reflections instead of two, on each ray before it reaches the slit, thus inverting the portion of each limb under examination, and bringing the very edges of the sun into juxtaposition, instead of a portion somewhat within the edge, as at present.

I have only to add in conclusion, that while the instruments used have been principally those of the Allegheny Observatory, to which institution my services belong, I should have been unable to carry out this research without certain indispensable adjuncts, the expense of which has been defrayed from an appropriation placed at my disposal by the trustees of the Bache fund.

Allegheny Observatory, April 14, 1877.